

Autobiography of Margaret Floy Washburn

Margaret Floy Washburn (1922)

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SOME RECOLLECTIONS

Nothing gives the writer of the following paper courage to present it but the fact that she herself can read with interest the autobiography of anything human. Even this thought is hardly relevant, for an account merely of one's intellectual life can hardly avoid depicting a prig rather than a human being. Nevertheless, the temptation not to be left out of the autobiographical enterprise is irresistible.

There are progressive persons, interested in educational theory, who love to describe the defects of their own early training, but I seem to remember chiefly what was helpful in mine, so that, like Marcus Aurelius, I begin my meditations by thanking the gods for having given me "nearly everything good."

I was born in New York City on July 25, 1871, in a house built for my mother's father. It stood surrounded by a large garden, on a tract of land belonging to my mother's maternal grandfather, Michael Floy, which originally extended from 125th to 127th streets and from Fourth to Fifth Avenues. At the time of my birth both sides of 125th Street from Fifth Avenue to the Hudson River were occupied by white-painted frame mansions set in gardens. This great-grandfather of mine came from Devonshire and had won success as a florist and nurseryman in old New York. I have reason to thank the gods for his



MARGARET FLOY WASHBURN

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diligence, which enabled me to finish my professional training without having to earn my own living. All my other ancestors were in America before 1720; one-fourth of them were Long Island and Westchester County Quakers, five-sixteenths New York Dutch, one-fourth Marylanders, and one-sixteenth Connecticut Yankees.

I was an only child, and the first eight years of my life were spent in the Harlem house; my father then entered the Episcopal ministry and for two years had a parish at Walden, an Orange County village. We next moved to the small Hudson River city of Kingston, where I got my high-school training and whence I went to Vassar.

It seems to me that my intellectual life began with my fifth birthday. I remember a few moments when I was walking in the gar-[p. 334]den; I felt that I had now reached an age of some importance, and the thought was agreeable. Thinking about myself was so new an experience that I have never forgotten the moment.

I was not sent to school until I was seven, but, like many other persons, I cannot remember the time when I could not read, nor when I learned. The first school was a private one kept by the Misses Smuller, the three accomplished daughters of a retired Presbyterian minister who lived in the next house. It would be hard to find better teaching anywhere at the present time. In my year and a half there I gained, besides the rudiments of arithmetic, a foundation in French and German that saved me several years in later life, and the ability to read music and play all the major and minor scales from memory, a musical grounding that has been the chief aid to one of my greatest sources of enjoyment.

When we left New York for the two-years' sojourn in Walden, my school was, though still a private one, much like the district-school type, housed in a single-room building. I learned very little there: some American history and a little elementary physics. During these two years, between the ages of eight and ten, I wrote stories, of which one or two examples remain. They display no literary talent whatever except a precocious vocabulary, due to my constant reading. A family legend, by the way, was that the subject of this autobiography, aged seven, having had a bad tumble at school and been established as an invalid for the rest of the day, described the behavior of a playmate in the following impressive terms: "And Enid stood rooted to the spot with amazement at beholding me comfortably established on the sofa." Besides children's books such as the immortal *Alice* -- in which the only thing I found funny was Alice's play with the black kitten before she went through the looking glass: the rest was highly interesting but not at all amusing --, George MacDonald's enchanting *The Princess and the Goblin*, which kept me awake the night of my seventh birthday and was read to pieces; all of Miss Alcott, Susan Coolidge, and Sophie May, I read between the ages of nine and twelve the whole of Dickens and the Waverley Novels.

The removal to Kingston came when I was eleven; here I entered a public school. By a blunder I was put into a grade too high for me, and suffered much anguish with arithmetic; in the spirit of M. Aurelius, however, it may be said that this was a piece of good fortune, for, managing somehow to scramble through the Regents' [p. 335] examinations, I entered the high school at twelve. New York State's system of Regents' examinations is, I believe, considered by all enlightened educators as below contempt, but I had much reason for gratitude to it. The terrifying formalities attending these examinations, where one's teachers with trembling fingers broke the seals on the packages of question papers sent from Albany, and one signed at the end of one's production a solemn declaration of having neither given nor received help, made all subsequent examinations in college and university seem trivial. What could be more comfortable and less awe-inspiring than being examined by one's own instructors?

The curriculum at Ulster Academy covered three years and would deeply distress a modern authority. It consisted of short-term courses in a large variety of subjects, each of which supplied a certain number of "Regents' credits." This method gave very poor results in the sciences, and my entire class failed twice to pass the Regents' examination in chemistry, having had no laboratory work. Our teacher performed some demonstration experiments, of which I can remember only sodium scurrying over the surface of water as a little silver ball and

potassium bursting into flame under similar circumstances; also Prince Rupert's drop falling into dust when its tip was pinched; why, we had not the slightest idea. However, the course in "political economy" firmly fixed in one's mind the rudiments of the theory of supply and demand, and that in "civil government" equipped one with some lasting idea of the structure of state, county, township, and city. We had to learn the Constitution of the United States thoroughly, and a few years ago I was able to impress my colleague of the Department of Political Science at Vassar by answering test questions on it. Passing Regents' examinations in Latin had somewhat the nature of a sporting event. Having read four books of Virgil, we tried the examination on all six, reading at sight the passages from the last two. Several of us got over this hurdle, and the *Aeneid* knew us no more. What we lost in literary appreciation was gained in confidence for sight reading.

During these years I read in all my spare moments. Never having to do any school work at home, and enjoying the blessed privilege of an only child to be undisturbed when at leisure, I devoured all of Thackeray and Fenimore Cooper, Irving, Don Quixote (illustrated by Doré), Cary's translation of the *Divine Comedy* (similarly adorned), and a wide range of other literature including *Gulliver's Travels*, Fox's *Book of Martyrs*, and what I could make out of the [p. 336] *Canterbury Tales*. There was a good library at home and another at the Academy. Scott was read and reread until I was fourteen, Dickens I read until about fifteen years ago, when his world began to seem too remote. That so much reading did no harm to health is shown by the fact that until I was twenty-six I was never ill. We had little in the way of out-door sport except skating, which came naturally to all dwellers by the Hudson.

In the spring of 1883 my parents and I made a memorable trip down the Mississippi from St. Louis to New Orleans by one of the old "palatial" steamers, which took a week for the run. I can still hear the call of the man with the lead, repeated from an upper deck and from the pilot-house, "Mark three!"; when it was "Mark twain!" a deep bell sounded once, the slow alternating puffs of the two engines stopped, and the great boat floated softly on over the shoal. The summer of my fourteenth birthday we went abroad for six weeks spent in the British Isles and in Paris; Walter Scott had made an excellent background for this journey.

I entered Vassar in the fall of 1886 as a preparatory student, for I lacked some Latin and had had no French since my earliest school days. Miss Smuller had laid so good a foundation that I needed only a semester at Vassar to secure admission to freshman French.

At this time there were no 'majors' in the Vassar curriculum. English, mathematics, Latin, a modern language, physics and chemistry, were required through the sophomore year; psychology, so-called, and ethics in the senior year; there was no requirement of continuity in any other subject. So far as there was continuity in my own studies, it lay in chemistry and French. Professor LeRoy Cooley taught chemistry and physics in crystal-clear lectures: his favorite word was 'accurate,' which he pronounced 'ackerate,' and I have loved, though by no means always attained, 'ackeracy' ever since. Particularly delightful was quantitative analysis, with the excitement of adding up the percentages of the different ingredients in the hope that their sum might approach one hundred; though the faint suspicion always remained that a particularly 'ackerate' result was due to losing a trace of something here and getting in a grain or two of dust there. French was admirably taught by two alternately kindly and ferocious sisters, Mlle. Achert and Mme. Guantieri, known to the students as Scylla and Charybdis: from the beginning no English was ever heard in the classroom, an unusual requirement in those days.[p. 337]

I am rather glad that I took no courses in English literature. When I was sixteen I began to love poetry, especially Keats, who absolutely bewitched me. Later, through a growing interest in philosophy, Matthew Arnold, with his matchless combination of classic beauty, clear thinking, and deep feeling became my favorite; I wrote my Commencement oration on "The Ethics of Matthew Arnold's Poetry," tracing the Stoic elements in it. For the love of poetry and philosophy I found in my sophomore year a strong stimulus in an older student who had been a senior in my preparatory year and had returned to college to work for a master's degree. She had been the leader of a brilliant group of girls in the class of '87, whose religious radicalism had

distressed President Taylor in his first year of office. I now experienced the mental expansion that comes with dropping orthodox religious ideas, an expansion accompanied by exhilaration.

From my junior course in English I remember gratefully a book called *Rhetorical Analysis*, by Professor Genung of Amherst. It consisted of selections of prose from a wide range of masters; at the bottom of each page were detailed questions on the style, which we answered in writing: such as, "Exactly what does each of these metaphors contribute?" "Why is 'which' used instead of 'that' here, although 'that' is more nearly correct?" This work was invaluable in developing the ability to say what one meant and I recall it every time I try to write.

A wonderful new field was opened in my junior year by a course in biology whose teacher was a young Bryn Mawr Ph.D., Marcella O'Grady. She later married Theodor Boveri, the great authority on cytology, and has now, some years after his death, returned to America and to teaching. She lectured admirably and drew beautiful figures on the board. In this year, too, I began the study of Greek: Professor Abby Leach was a skillful teacher of its grammar, and brought the little group of my classmates in two semesters to the point where they could join the incoming freshmen who had had two years' preparation. I cherish proudly the scraps that remain, and pity the person who has to master scientific terms with no knowledge of Greek.

It was, I think, the summer after my junior year that I read in my father's library Arthur Balfour's *Defence of Philosophic Doubt* and acquired for a lifetime the conviction that no one has ever succeeded in constructing a logic-proof system of monistic metaphysics.[p. 338]

President Taylor's course in psychology, required in the first semester of all seniors, was based on James Clark Murray's *Handbook of Psychology* and lectures on the history of philosophy by Dr. Taylor. Murray's book was directed against the associational school, Dr. Taylor's lectures against materialism. Murray's argument was that association could not explain the process of active relating, which he called comparison: "association can merely associate." This was a sound position: James had expressed the same thing the year before in pointing out the neglect of 'selective attention' by the associational school. The problem is focal in psychology at the present time, with the believers in 'creative mind,' vitalism, voluntarism, and so forth on one side and the mechanists on the other: I firmly believe that it can be solved by mechanism, but not that of the old associative type. Dr. Taylor's attacks on materialism were made, not from the idealistic point of view, but from that of 'natural realism,' originating with Reid and still being defended in those days by President McCosh of Princeton. Dr. Taylor (whom, by the way, we regarded with great affection) had no idea of presenting metaphysical systems to us impartially: he wished to preserve our religious convictions by saving us from materialism in the one direction and pantheistic idealism in the other. This vigorous special pleading was more stimulating than the most conscientiously impartial presentation of opposing views could have been.

At the end of my senior year I had two dominant intellectual interests, science and philosophy. They seemed to be combined in what I heard of the wonderful new science of experimental psychology. Learning of the psychological laboratory just established at Columbia by Dr. Cattell, who had come a year before from the fountain-head, the Leipzig laboratory, I determined to be his pupil, and my parents took a house in New York for the year. But Columbia had never admitted a woman graduate student: the most I could hope for was to be tolerated as a 'hearer,' and even that would not be possible until after Christmas when the trustees had met. I solaced myself by taking the School of Mines course in quantitative chemical analysis at the Barnard laboratory, the second floor of a brownstone house on Madison Avenue. President Butler was then the amazingly efficient young dean of the department of philosophy, and, at his suggestion, I read Wundt's long article on psychological methods in the first volume of *Philosophische Studien*; having had only a year of German, I began by writing out a translation of it, an excellent [p. 339] way of getting the vocabulary. After Christmas I was allowed to present myself to Dr. Cattell for admission as a hearer. The psychological laboratory was the top floor of the old President's House on Forty-ninth Street close to the New York Central tracks. "What do you think is done in psychological laboratories?" asked Dr. Cattell, who looked then just as he does now, barring the grey hair. I

blessed the hours I had spent on W. Wundt's article: instead of speaking as I am sure I was expected to do, of hypnotism, telepathy, and spiritism, I referred to reaction-time, complication experiments, and work on the limens and Weber's Law, and was rewarded by the remark that I seemed to have some knowledge of the matter.

From that time Dr. Cattell treated me as a regular student and required of me all that he required of the men. A lifelong champion of freedom and equality of opportunity, it would never have occurred to him to reject a woman student on account of her sex. The four men students, seniors, and I listened to his lectures, prepared reports on experimental work, and at least one paper on a theoretical subject. He assigned to me the experimental problem of finding whether Weber's Law held for the two-point threshold on the skin. I improvised apparatus, used a metronome to keep the duration of the stimuli constant, and found observers among my Barnard associates. Incidentally, it may be mentioned that Weber's Law does not hold for the two-point threshold. I also exchanged hours of observation with Harold Griffing, the only graduate student, who was engaged on his thesis, *On Sensations from Pressure and Impact*. He was a man of great promise, heavily burdened with the support of his invalid mother and sisters; he died a year or two after taking the doctorate. He would have been a leader in American psychology.

Dr. Cattell raised me to the height of joy after I had read a paper on the relation of psychology to physiology by writing me a note to suggest that I send it to the *Philosophical Review*. Nothing would have induced me to do anything so daring. At the end of the year, since there were no fellowships at Barnard, he advised me to apply for a graduate scholarship at the newly organized Sage School of Philosophy at Cornell. I feel an affectionate gratitude to him, as my first teacher, which in these later years I have courage to express; in earlier times I stood too much in awe of him.

While I was thus being initiated into Cattell's objective version of the Leipzig doctrine, the influence of William James's *Principles* was strong. His enthusiasm for the occult was unattractive; it [p. 340] seemed that in his zeal to keep an open mind he kept it open more widely to the abnormal than to the normal. But his description of the stream of consciousness, and the consistently analytic rather than synthetic point of view which he maintained in holding that simpler mental states are products of analysis, and in developing all spatial relations by analysis from a primitive space instead of compounding them like Wundt out of non-spatial elements, never lost their effect even though the prestige of the Leipzig school increased.

I went in the fall of 1892 to Cornell, where Titchener had just arrived from Oxford and Leipzig. He was twenty-five, but seemed older at first sight because of his square-cut beard; the illusion of age vanished on acquaintance. There was nothing about him at that time to suggest either his two greatest gifts or his chief failing in later life. The gifts, in my opinion, were his comprehensive scholarship, shown conspicuously in his *Instructor's Manuals of Experimental Psychology*; and his genius as a lecturer. In his first two years at Cornell his lectures were read, and were frankly after the German fashion: we regarded him as a brilliant young man who would give us the latest news from Leipzig, rather than as one to be heard for his own sake. The failing that later grew upon him was that of remaining isolated so far as his immediate surroundings were concerned from all but subordinates. In these first years he was entirely human. He once asked me to look over some proof; finding a sentence whose meaning was obviously inverted, I asked, "Didn't you mean" so-and-so? "Of course I did, ass that I am!" was the hearty response, a response that I fancy would have come far less heartily a few years later.

I was his only major graduate student, and experimental psychology was so young that he did not quite know what to do with me. Appointments for planning laboratory work would be made which often ended in his telling stories of Oxford life for an hour or two. He finally suggested that since I had some experience in work on tactual space perception, I should make an experimental study of the method of equivalents. I wrote up the results in a paper which was accepted in June at Vassar for an M. A. *in absentia*, Titchener having given me a written examination lasting three hours, of which I do not recall a single question. The paper was next

year incorporated into my doctor's thesis, and was resurrected a few years ago by Gemelli in his study of the method.

The Sage School of Philosophy was an inspiring place to work, for [p. 341] the members of its faculty were nearly all young. I chose as my minor subjects philosophy and ethics. President Schurman taught the advanced course in ethics. He had visited Vassar in my senior year and given several lectures on Herbert Spencer, which it was my privilege to report as a college editor. They were models of clearness and force. I have always greatly admired him, and it is a keen pleasure to read of his diplomatic triumphs at an age when most men are resting on earlier laurels. To be his pupil was a privilege. I had also a course with Ernest Albee in Leibnitz, Hume, and Kant, and with William Hammond in Greek and mediaeval philosophy, and read Kuno Fischer with Frank Thilly one hour a week for drill in philosophical German. Among my fellow-students were Joseph Leighton, now of Ohio State University, Edgar Hinman of the University of Nebraska, Albert Ross Hill, later President of the University of Missouri, Melbourne S. Read of Colgate, and Louise Hannum, a remarkably able woman who afterwards taught in Colorado.

At the end of this year I was asked to take the Chair of Psychology at the Woman's College of Western Reserve University, and went to Cleveland to look it over. The opportunity was a good one, but I think I was wise in deciding to finish my work for the doctorate at Cornell, although Dr. Schurman disapproved of the decision. In my second year at Cornell I was no longer Titchener's only major student, being joined by Walter Pillsbury from Nebraska; this is an association of which I have always been proud. I had, during my work with the method of equivalents, thought of a subject for a doctor's thesis: the influence of visual imagery on judgments of tactual distance and direction. Much of my time this year went to the thesis. I had also a course in Lotze's metaphysics with F. C. S. Schiller, who had come from Oxford for a year's stay in the wilderness and was even then a very distinguished man. The thesis was finished by the spring vacation, and Dr. Titchener sent it to Wundt, who had it translated into German and published in *Philosophische Studien*, where the Leipzig theses appeared. On this occasion my translator enriched the German language with a new verb: *visualisiren*.

Examinations for the doctorate at that time were wholly oral. Some of the questions at mine I can recall. Dr. Schurman quizzed me on Spencer's *Data of Ethics*, which was a piece of luck for me, since in my senior year I had mildly annoyed Dr. Taylor and secured [p. 342] intervals of repose for my classmates by quoting it extensively. Dr. Titchener asked me something about Müller and Schumann's work on lifted weights, and also a question which I could not answer: the correct answer would have been, "The cornea," but why the cornea I have no recollection. Dr. Creighton asked me to name the Kantian categories, and what the relation of the third one in each group was to the other two; also about Berkeley's theory of causation. Dr. Hammond wished to hear about philosophy in the ninth century. The occasion was a pleasant one. I received the doctor's degree in June, 1894.

No position was waiting for me, and I even considered teaching psychology in a New York finishing school. The elderly gentleman at its head impressed me with its high standards: all the members of his senior class in astronomy the last year had attained the mark of 100 per cent. Before I committed myself to this institution a telegram asked me to come to Wells College. Its new president, Dr. William E. Waters, being a classical scholar, preferred to teach Greek instead of the psychology and ethics required of a college president; in this emergency they could offer me little money, but I gladly accepted the Chair of Psychology, Philosophy, and Ethics (not to mention logic), at a salary of three hundred dollars and home. (The arrangement with my family was that when I visited them they paid the expenses of importation, but I must pay my own way back). Wells was, and I hear still is, though much grown, a delightful place; I spent six years there that left not a single unpleasant memory. The salary, by the way, had by the last two years reached the maximum for women professors, seven hundred dollars and home; the men were paid fifteen hundred. What money meant in those days is shown by the fact that at the end of the six years I had saved five hundred dollars without any effort at all. During several of these years I spent one day a week at Cornell. Titchener was already withdrawing from contact on equal terms with his colleagues in the Sage School, who went

their own way, and, as they were my especial friends, I saw little of the Director of the Laboratory, though he was always kind and helpful when we met. I published during this time some observations on afterimages and two or three articles on other subjects which may remain forgotten. Late in the summer of 1897 I fell ill with typhoid during a visit in Vermont, and could not return to work until the first of December. Without my knowledge, Professors Creighton [p. 343] and James Seth had taken charge of my courses, coming down twice a week from Ithaca; thus my misfortune was great gain for my students.

During this period I accepted the general point of view of what Titchener called structural psychology. To a person with a liking for chemistry the idea of introspectively analyzing mental states into irreducible elements had attraction, yet one could not forget James's conception of consciousness as a stream and the impossibility that it should be at once a stream and a mosaic. I never followed Titchener when he developed his elaborate, highly refined introspective analysis, and not one of the doctor's theses produced at Cornell and later at Clark (under Baird) by the use of this method had any real appeal for me. It is worth while to describe conscious states, but not, in describing them, to turn them into something unrecognizable. Münsterberg's work was attracting attention. I liked the theory of knowledge which he developed in his *Grundzüge*, that brilliant book which he never finished, preferring to waste his great powers in writing articles for American popular magazines. He was a dualist but not an interactionist, a position which perfectly suited my own skepticism with regard to monism. He restricted causality to the series of physical events, and regarded psychic processes as epiphenomena; this still seems to me the only defensible position. His own method of structural analysis, however, which sought a psychic Ur-element as the accompaniment of the activity of a single cortical neuron, was indefensible psychologically, physiologically, and psychophysically; that is, his introspective analysis was fallacious; his physiological hypothesis that single cortical neurons act alone was highly improbable, and there was not the slightest evidence that his mental Ur-element was the accompaniment of such a physiological Ur-element! But his emphasis on movement as an explanatory concept seemed to me highly promising. Structural psychology was weak on the explanatory side: motor processes could help it out.

In my sixth year at Wells I became restless, and felt that a year at the Harvard laboratory would be a refreshing change. I was granted leave of absence for this purpose in the spring of 1900, but a telegram from President Schurman changed my plan. He asked me to come to Cornell as Warden of Sage College, with plenty of opportunity for my own psychological work and what then seemed the enormous salary of fifteen hundred dollars and home. So I returned to Cornell for two years. In the first, I tried to work out [p. 344] in the physics laboratory the problem of the flight of colors, but did not succeed in obtaining good results from any controllable source of light. I had to spend too much time and energy at social functions, which, however, gave much profitable experience in other directions. In the physics laboratory I served as an observer for Frank Allen's research on the fusion rate of retinal impressions from different regions of the spectrum, and got a further glimpse of the futility of elaborate introspection. As I observed and reported on the visual phenomena, I accompanied my judgments of fusion by introspective accounts of variations in my general state of mind which would undoubtedly make the curves in one experiment quite different from those in another. I mentally congratulated Mr. Allen in having for the first time an observer skilled in introspecting sources of error. Much surprise resulted when the curves proved highly uniform; the sources of error had not influenced the sensory judgments at all.

Moreover, I was now impressed with the lack of agreement among structural system-makers in regard to conscious elements and their attributes. I wrote a paper for Professor Titchener's seminar, which I occasionally attended in 1900-1901, comparing the structural systems of Wundt, Ebbinghaus, and Münsterberg; and showing how completely they were at variance in their conceptions of element and attribute. Titchener reacted strongly against this paper: I cannot remember what his specific objections were, but I realized the awe in which he was now held by his students when a member of the class came to me next day with sympathy for his 'injustice,' which no one had ventured to point out the night before! It had not occurred to me to be depressed by Titchener's criticisms; it was exciting to 'draw blood' from him. The paper appeared in Volume XI of the *Philosophical Review*, and I later found that it was welcomed by

some of the rising school of functional psychologists as helping to demonstrate the uselessness of structural psychology.

In the second year of this later sojourn at Cornell I was appointed, with Titchener's very kind and cordial consent, a lecturer in psychology, and gave a course in social psychology based to some extent on Wundt's *Völkerpsychologie* (the volumes on speech were the only ones published at that time); and a course in animal psychology. My colleagues in the Department were Bentley, Whipple, and Baird, and one of my pupils was Robert Gault.

I wrote in this year another paper which indicated departure from Titchenerian doctrine; it tried to show the impossibility of regarding duration as an attribute of sensations.[p. 345]

Since this dissatisfaction with extreme structuralism was accompanied by a growing interest in motor processes, it might seem that functional psychology, now coming to the fore, would have been a refuge. Several influences kept me out of it. For one thing, it was the child of pragmatism, which was itself sponsored by Dewey and James. Professor Dewey is undoubtedly one of the few great leaders of American thought, and I have felt the charm of his character and personality, but, through some congenital disability, I cannot read him. And James, despite the enduring influence of his psychology, as a philosopher inspired me with distrust. The doctrine that ideas are true in proportion as they 'work' may too readily be used to mean that they are true in proportion as they are comfortable. Moreover, as Bentley pointed out in a review of Angell, after abandoning the description of conscious processes, functional psychology had little left to say except to show how they served 'the welfare of the organism.' Also, if it really meant that mental processes *as such* had significance for bodily welfare, this was interactionism, and I could not and cannot tolerate it.

Being a 'warden' and having to concern oneself with the behavior of other people was highly uncongenial, and when, at the end of two years, I was offered an assistant professorship in full charge of psychology at the University of Cincinnati, where Dr. Howard Ayers was President, I eagerly accepted it, though I disliked going so far from my parents. I was the only woman of professorial rank on the faculty, and President Ayers took especial pains to treat me, as we sat around a long table at faculty meetings, on a footing of perfect equality with the men. Judd had just resigned the charge of this laboratory to go to Chicago. My colleagues were an exceptionally able group, including, for example, Michael F. Guyer in biology, Louis T. More in physics, and F. Hicks, later President of the University, in economics. A drawback at Cincinnati was the quality of the student material. The University was compelled to admit all graduates of city high schools, and at the end of my first term I had to condition half my introductory class. The place offered, however, many opportunities, but it is hard for a deeply rooted Easterner to be transplanted. When I sat in the station and heard the train called for "Buffalo-Rochester-Syracuse-Albany," the sound was sweet in my ears, and I can still remember the thrill of happiness that came with the first stir of the car wheels on their eastward journey. I was thankful when President Taylor in the spring of [p. 346] 1903 called me to Vassar as Associate Professor of Philosophy. There I could spend every Sunday with my parents, who were living only sixteen miles away.

Psychology at Vassar since Dr. Taylor gave it up had been taught by the Professor of Philosophy, H. Heath Bawden, a pupil of Dewey's and an inspiring teacher, who had made considerable reputation as a member of the pragmatist group. He had established the beginnings of a psychological laboratory in the basement of the building dedicated to biology. All the work in psychology was turned over to me, with the addition of a semester's course in ancient philosophy and one in modern philosophy. No one will care to read an account of the progress of courses in psychology at Vassar: it is enough to note that an independent department was formed in 1908 and that the laboratory now occupies more than half of a three-story and basement building.

In order to give the senior students in psychology a glimpse of research methods, a few simple experimental problems were devised each year, whose results, if they worked out successfully, appeared in the *American Journal of Psychology* as "Studies from the Psychological Laboratory

of Vassar College." The problem and method of a study having been determined by me, the experimenting was done by the students, who also formulated the results; the interpretation and writing of the reports fell to me and the paper was published under our joint names. Fifty-seven such studies have appeared. Only three times have we conferred the master's degree in psychology; the college does not accept candidates for the doctorate. Personally, I deprecate graduate study for women at any but coeducational universities.

In my first year at Vassar (1903-1904) I wrote a short article developing the idea that both the capacity of making sensory discriminations and that of recalling memory ideas have been dependent on the possibility of delaying reaction, a possibility which arose from the development of distance receptors. Two years later the importance of these receptors for mental development was pointed out by Sherrington in *The Integrative Action of the Nervous System*. In 1904 I read before the Psychological Association a paper showing some of the difficulties involved in the Wundtian tri-dimensional theory of feeling, and expounding the idea that feeling is the unanalyzed remainder out of which sensations have emerged; the power of analysis having been earliest developed where it was most needed,[p. 347] that is, with reference to outside stimuli. The genetic point of view was much in my mind during these years, and so were kinaesthetic processes. To the Stanley Hall *Festschrift* in 1903 I contributed the suggestion that the social reference of certain conscious states, e.g., the thought of another's suffering, had as its nervous basis kinaesthetic processes from certain incipient reactions, for instance the impulse to help. At a symposium on the term 'feeling,' held at the 1905 meeting of the Association, I defined feeling as the unanalyzed and unlocalized portion of experience, and suggested that James's feelings of 'but' and 'if' might be the remnants of ancestral attitudes. James was present and approved of the idea.

If this were an emotional instead of an intellectual autobiography, an almost morbidly intense love of animals would have to be traced to its occult sources. Animal psychology began to occupy me when I gave a course on it at Cornell. During a six-weeks' stay at Ithaca in the summer of 1905, I collaborated with Dr. Bentley in some experiments on color vision in a brook fish which he captured from a neighboring stream. The chub learned with great speed, in spite of lacking a cortex; it discriminated both dark and light red from green. Our method of eliminating the brightness error by varying the brightness of the red was inadequate, but later investigators have confirmed our results. Shortly after this, I began to collect and organize literature on animal behavior. The Animal Behavior Series, which the Macmillans published under Dr. Yerkes' editorship, brought out the first edition of *The Animal Mind* in 1908. While the objective school of interpretation, represented in America chiefly by Loeb, had long urged that much animal conduct should be regarded as unaccompanied by mind, no one had then suggested that all animal behavior, still less that all human behavior, is unconscious, and the patterns of animal consciousness seemed to me then, as they do now, well worth investigating and perfectly open to investigation.

The experimental study of thought processes that was now being carried on by the so-called Würzburg School suggested a further use for kinaesthetic explanations, and in 1909 I wrote a paper on "The Physiological Basis of Relational Processes" which developed more fully the idea propounded at the 'feeling' symposium, and maintained that relational processes in general are close fusions of kinaesthetic excitations. The arguments for this were that kinaesthesia accompanies all other sensory experience and is a suitable basis for relational processes, which are common factors in different sensory situa-[p. 348]tions; that many relational processes are for introspection accompanied by kinaesthesia from attitudes; and that where the relational processes are unanalyzable into kinaesthetic sensations, this may be due to the fact that the attitudes concerned are phylogenetically very old, and that there has been little practical need for such analysis -- the point made in 1904.

The influence of the Würzburg School and of Ach also brought to the fore in the psychology of the time the problem of purpose, the *Aufgabe*. Structural psychology had overlooked this in its strong bent towards reducing everything to sensations. I was enough of a structural psychologist to seek for sensational explanations if haply they might be found, for the reason expressed in the 1909 paper just mentioned: "The sensory process is one about which we

know more, it is of a less conjectural and made-to-order character, than other hypothetical cortical processes." That the basis of an *Aufgabe* should lie in an attitude seemed especially possible, since the essence of a purpose lies in its tendency to persist, and attitudes are characteristically persistent as compared with movements and with sensory processes. At the 1913 meeting of the Association I presented the following ideas: "An essential characteristic of an *Aufgabe*... is that it associates with itself a bodily attitude which may be called the activity attitude. The *Aufgabe* may drop out of consciousness and still influence associative processes if the organic-kinesthetic fusion resulting from the attitude remains." This doctrine of a persistent bodily attitude as the basis of purposive thought and action has strengthened its hold on my mind since its first formulation.

In the next year, the fundamental principles of a motor system of psychology were laid down in an article entitled *The Function of Incipient Motor Processes*. It explained association as essentially the association of movements, a doctrine based on the fact that impressions do not become associated merely by occurring together, but only if they are attended to together, attention being regarded as a motor process. It also presented a physiological theory of the image or centrally excited sensation. "If a motor response is initiated" and antagonistic excitations delay its full performance, "all the sensory centers that have recently or frequently discharged into the motor center concerned . . . are set into excitation," this process being accompanied in consciousness by imagery. The idea of incipient or tentative movements had been vaguely in my mind since 1903, when in the Hall *Festschrift* article I made use of "sensations resulting from the stirring of an impulse." [p. 348]

During all those years most of my vacation time was spent with my parents. The family fortunes having declined, they were living at Newburgh, enjoying a superb view of the Hudson but little variety, and I was disinclined to leave them for long. In the summer of 1906, while working on *The Animal Mind*, I spent three weeks at Cambridge looking up material in the Museum Library, and had an opportunity to see Professors James and Royce at their homes. From 1913 to 1917 I taught in the Columbia Summer School and with great pleasure and profit came to know Woodworth, the Hollingworths, Poffenberger, and the Montagues. I occupied Dr. Cattell's office, opening out of Dr. Woodworth's, and admired the chances of fortune that had raised me so high. In December, 1914, my father died and my mother came to live with me at Vassar until her death in 1924. Both my parents always took pleasure in my work.

I had for some time been collecting the results of all the German and French experiments on the higher mental processes. Vassar celebrated in 1915 the fiftieth anniversary of its founding, and the trustees decided to publish a commemorative series of volumes by alumnae, books of a scholarly rather than popular nature, which might not readily find a publisher in the ordinary way. For this series I wrote *Movement and Mental Imagery*, trying to interpret the experimentally obtained data on the higher mental processes by the motor principles I had been evolving, and developing the doctrine that thinking involves tentative or incipient movements. Since the series was published in so uncommercial a fashion, it got little advertising, but at least I can say that '*M. and M. I.*' has considerably increased its sales within the past three or four years. I shall never cease to be pleased that Hollingworth read it when it came out and spoke kindly of it, not to me but to his wife: a second-hand compliment has double value. Another person whose praise of it will always be remembered is Professor T. H. Pear, who reviewed it for the *British Journal* and discussed it in his *Remembering and Forgetting*.

In 1917, I wrote for Titchener's *Festschrift* an article which had nothing to do with the motor theory, but presented some ideas on which I had been basing a course in social psychology ever since 1902. The concept of *ejective consciousness* had proved itself useful in analyzing and classifying the phenomena of social relations. The term was borrowed from W. K. Clifford's 'eject,' by which he meant [p. 350] a state in another person's mind, and ejective consciousness was used to designate awareness of processes in other minds. Thus, while both man and lower animals act socially, man has a much greater tendency to think about what is going on in the minds of his fellow-beings than the animals have, and this tendency brings about characteristic modifications in social behavior. The term is not a motor one, but in the Hall *Festschrift* fourteen

years earlier I had suggested a motor basis for ejective consciousness. In social psychology it seems more convenient to use the concept without discussing its physiology. It is serviceable in discussions of language, religion, and art, and is still employed in my social psychology course.

Watson's radical behaviorism was of course the favorite topic of discussion in the years from 1915 to 1922 or thereabouts. It will be remembered that his first attack on the existence of conscious processes consisted in denying that of mental imagery. A critic could easily point out that his principles required also denial of the existence of all sensation qualities. In fact, the existence of sensation qualities is irreconcilable with any materialistic monism. My presidential address before the 1921 meeting of the Psychological Association tried, while rejecting the Watsonian metaphysics, to show that introspection itself is an objective method and one necessarily used by the behaviorist.

The evening of that address was one of perfect happiness for the speaker, whatever the sufferings of the audience may have been. The scene was the beautiful Gothic dining-hall of the Princeton Graduate School, and I sat at the high table on the dais. At intervals during the banquet strains from the fine organ, under the skillful hands of Dr. Carroll Pratt, rose to the carved beams of the roof. I had rewritten my speech so many times that it was as good as I could make it and was dismissed from my thoughts. Just before we sat down, Dr. Walter Bingham had completely surprised me by saying that before my address he would announce the award of the prize of five hundred dollars offered by the Edison Phonograph Company for the best research on the effects of music, to a study made by my colleague Dr. George Dickinson of the Vassar Department of Music and myself on "The Emotional Effects of Instrumental Music." By the way, Dr. Bingham's memory played him false when, in his introduction to the volume, *The Effects of Music*, which contained the papers submitted in the contest, he stated that the prize was given to the other study which I had submitted, in collaboration with Misses Mead and Child.[p. 351]

At this meeting of the Association a Committee on the Relation of the Association to Publication was appointed, consisting of Messrs. Langfeld, Franz, and myself as Chairman. A previous committee under the chairmanship of Judd had been formed in 1920 and had been unable to present a report. The subject in hand was the development of a *Journal of Psychological Abstracts*, and the difficulty was as follows. Professor Warren, the owner of the *Psychological Review* publications, was conducting an abstract journal in the form of certain numbers of the *Psychological Bulletin*. There was a desire on the part of many psychologists that such a journal should be enlarged, which could not be done without a considerable subsidy; but no subsidy could be obtained, for example, through the National Research Council, so long as the journal was privately owned. At the 1922 meeting of the Association, our Committee reported that Professor Warren had offered to give the Association an option to buy the fifty-five shares of the Psychological Review Company's shares at fifty dollars per share, *plus* the accumulated unpaid dividends since the incorporation of the company, amounting to 42 per cent of the purchase price. Thus the Association would become the owner of all the publications. The Association authorized our Committee to see that the option was drawn up. On December first, 1923, Dr. Cattell, Chairman of the National Research Council's Committee on Psychological Abstracts, asked Mr. Langfeld and me to meet his Committee; we accordingly did so, and reported to the 1923 meeting that the Psychological Division of the National Research Council would try to obtain funds for the establishment of an independent Abstract Journal if the Association would vote to take up the option for the purchase of the *Psychological Review* journals and appoint a committee on ways and means of paying for them. The Association did so, appointing our Committee to continue in this function. In the spring of 1924 Mr. Langfeld and I met with Messrs. Anderson and Fernberger, the Association's Secretary and Treasurer, and a plan was formed which involved gradually raising the annual dues to ten dollars and paying off the debt by notes falling due in successive years. Meantime, Professor Warren had generously waived the matter of the unpaid dividends. This plan was laid before the members of the Association by mail for an expression of opinion, which was favorable by a large majority, and at the 1924 meeting the project was adopted. Meantime, I had been appointed by our Division of the National Research Council chair-[p. 352]man of a sub-committee to secure a subsidy for the projected Abstract Journal, the other member being Professor Stratton. We met with a committee from the Association, consisting of Professors Langfeld, Fernberger, and

Hunter, and made out a budget, with Dr. Cattell's advice, for the new journal, requiring a subsidy of \$76,500, which was subsequently obtained from the Laura Spelman Foundation. At the 1928 meeting of the Association, Professor Warren cancelled the remainder of the Association's debt to him.

By the way, the unreliability of having the Association express its opinion by mail was comically shown at this meeting. A project for the certification of consulting psychologists which I reported as having been favored by a considerable majority of the Association when presented through the mails was turned down vociferously by the psychologists present in the flesh. The mail canvass is, however, valuable for the publicity it gives a project.

During the next five years I attempted, with the aid of my students, various small studies in the difficult field of emotions; also, with the help of a grant from the American Association for the Advancement of Science, a questionnaire study on sources of pleasure, anger, and fear in groups of Italian and Russian Jewish women in New York and Chicago.

When the Division of Anthropology and Psychology of the National Research Council was constituted I was a member for the first year, but was not re-elected; in 1924, I was elected by the Psychological Association for a three-year period. While Dr. Stratton was chairman of our Division of the National Research Council, he established a committee on the experimental investigation of emotion, appointing me chairman and Drs. Dodge and Dunlap as the other members. I do not think my fellow-members were hopeful as to the prospects of this committee, whose object was to discover ways and means, including funds, for such investigation. In fact, when I arrived at our first committee meeting I found one of them on the point of resigning. He consented to remain, and we called a conference of experimental workers in the field of emotion at Columbia on October 15 and 16, 1926. It was attended by Messrs. F. H. Allport, Blatz, Brunswick, Dodge, Dunlap, Gesell, Landis, Moore, Nafe, Wells, and Woodworth, besides several others who came in for an hour or two. Our committee presented to the next meeting of the Council a report recommending the support of researches by Lashley and Landis. Unfortunately, at this time the Laura Spelman Foundation, the chief source of funds for our Division of the National Research Council, announced its intention of supporting no more individual projects, so the pessimism of my fellow-members was justified.

In the autumn of 1925 Titchener resigned the editorship of the *American Journal of Psychology* on account of a difference of opinion with Dallenbach, its sole owner, with regard to the ultimate disposal of the property. Dr. Titchener was, I am convinced, in the wrong, and the breach was a grief to Dr. Dallenbach, who had felt for him a most loyal affection. At the Christmas meeting of the Association that year, Bentley, Boring, Dallenbach, and I discussed in the study on the top floor of the Dallenbach house until midnight the future of the *Journal*. When we descended to the wives of these gentlemen, patiently waiting before the living-room fire, we had agreed to edit the *Journal* jointly, and thus began an association which has been unclouded by a single disagreement or unpleasant feeling. Dr. Dallenbach bears with conspicuous ability the heaviest burden of the work.

A second edition of *The Animal Mind* had appeared in 1917, nine years after the first, and a third one seemed to be due in 1926; this time the book was very considerably rewritten. Reviewing Woodworth's *Dynamic Psychology* in 1918, I began to realize how completely my motor theory had ignored the explanatory function of the drive. Of course, one had taken for granted that an animal would not learn without a motive, but, as I analyzed in 1926 the recent literature on learning, especially the work of Szymanski, it became clear that the drive explains the formation of successive movement systems by being present throughout the series, and by setting in readiness its own consummatory movements. A paper on "Emotion and Thought," written for the Wittenberg Conference on Feelings and Emotions, discussed some relations between the passage of drive energy into visceral and non-adaptive muscular movements, as in emotion, and into tentative movements and the 'activity attitude,' as in thinking. In my address as retiring Chairman of Section I of the American Association for the Advancement of Science, December, 1927, I used the passage of a drive into the activity attitude as a mechanistic explanation of purposive action, and urged that vitalism and emergent evolution, in

general, are too ready to adopt the primitive mind's recourse to unknown forces. The address also suggested that a precursor of the activity attitude might [p. 354] be the 'orientation towards a goal' observed in animals learning a maze path; this idea was further developed and some experimental results, showing the influence on maze orientation of the presence of food during the running and of the initial run's direction, were presented at the meeting of the Ninth International Congress in September, 1929.

The enthusiasm with which the *Gestalt* psychology was being preached in America during these years by Köhler and Koffka was far from being unwelcome; it was a real pleasure to have the patterns of consciousness, surely among the most fascinating objects in the universe, made the subject of thorough study and experiment instead of being stupidly ignored after the behaviorist fashion. It did, however, appear that configurationism was inclined to take vocabulary for description and description for explanation, and might well be supplemented by motor principles. At the 1925 meeting of the Psychological Association I suggested how the nature of the motor response could be used to explain certain phenomena of perception which are fundamental in the *Gestalt* doctrine, and, in a round-table discussion at the International Congress meeting in 1929, I made a similar suggestion in regard to association. Köhler, in replying, said among other things, "Why should we be expected to explain? Why is it not enough for the present to describe" or words to that effect. This delighted me, for I had expected him to say, "We configurationists have a thoroughly adequate principle of explanation, but unfortunately Miss Washburn is unable to understand it!" Which would have been unanswerable, because, in its latter portion, quite true.

The results of experimental work, if it is successful at all, bring more lasting satisfaction than the development of theories. Some of the small studies from the Vassar laboratory which have covered a period of twenty-five years do give me a measure of such satisfaction, to wit: certain observations on the changes occurring in printed words under long fixation; the fact that the movements of the left hand are better recalled than those of the right, probably because they are less automatized; the fact that movement on the skin can be perceived when its direction cannot; observations on the perception of the direction in which sources of sound are moving; observations on retinal rivalry in after-images; a study of the trustworthiness of various complex indicators in the free association method; experiments on the affective value of articulate sounds and its sources; the [p. 355] concept of affective sensitiveness or the tendency to feel extreme degrees of pleasantness and unpleasantness, and the fact that it appears to be greater in poets than in scientific students; the first experiments on affective contrast; the fact that the law of distributed repetitions holds for the learning of series of hand-movements; the study of revived emotions. In 1912, Miss Abbott and I proved red color-blindness in the rabbit, and, incidentally, that the animal reacts to the relative rather than the absolute brightness of colors, a fact later exploited by the configurationists. A student, Edwina Kittredge, proved that a bull-calf also was red color-blind; this coincided in time with Stratton's disproof of the notion that red angers bulls. In 1926, I published a study on white mice, in which, measuring activity by the actual speed of motion in the maze, and hunger by the time spent in eating, the effects of hunger and those of the impulse to activity were separated; another feature of this study was that each mouse's results were treated individually.

The Wittenburg Conference on Feelings and Emotions, October 19-23, 1927, was a remarkable affair. The readers of this article all probably remember how the wonderful energy and efficiency of Dr. Martin Reymert, with the enlightened support of the college administration, made the opening of a psychological and chemical laboratory at a comparatively small Ohio college a truly international event. It is likely that other speakers beside myself arrived at Springfield wondering whether any one but ourselves would be present to hear us. Dr. Carr told me while we waited in the crowd outside the assembly room that he had telegraphed Dr. Yoakum not to make the long journey up from Texas, as the conference might not justify the trouble; the latter disregarded this advice and came. In fact, the conference was worth far more than any meeting of the Psychological Association, since many of the leading psychologists of Europe and America sent or presented papers, and the audiences must have averaged five hundred psychologists. I had an odd experience at the close of the discussion of my paper. A fiery black-haired member of Congress startled the audience by rising from the front seats and fiercely challenging something I had been quoted as saying in a newspaper interview the

previous day about the superiority of education over legislation as a means of reform. He reminded one so vividly of a statesman out of *Martin Chuzzlewit*, and the size of the disturbance he made was so comically out of proportion to the insignificance of its cause that one could not help enjoying the incident. Later I was [p. 356] told that he was seeking re-election. On the last day of the conference honorary degrees were conferred upon some of the chemists and psychologists; my being included was, I was sure, due to my having been the only woman speaker, but I liked being photographed standing between Dr. Cattell and Dr. Cannon.

The Christmas holidays of this same year were also full of excitement. A year previously Dr. Dallenbach had written me of the *Journal's* plan to publish a volume in my honor commemorating the end of a third of a century of psychological work. The project was to have been a secret until carried out, but "Boring's Quaker conscience" felt that I should be warned. I was quite overwhelmed at the prospect.

The Christmas meeting of the Association in 1927 was at Columbus, Ohio. As I was not well, and had to go to Memphis to give the address of the retiring Chairman of Section I of the American Association for the Advancement of Science, I decided to omit the Psychological Association meeting; but on learning that my Journal colleagues planned a dinner for me at Columbus, my duty and inclination were alike plain. On Wednesday evening I spoke at Memphis before a joint dinner for Sections H (education) and I; Dr. Haggerty, as the retiring Chairman of Section H making the other address. There were not more than forty persons at the dinner, and they were all educators, quite uninterested in what I had to say about purposive action; all the psychologists had left for Columbus except Mr. and Mrs. Gates, to whom I addressed myself with pleasure. The great advantage, however, which the section chairmen of the American Association for the Advancement of Science enjoy is that of having their addresses printed in *Science* and thus reaching the finest scientific audience in the world: I had later several interesting letters from men in other fields who shared the mechanistic point of view of the paper. At the close of the dinner I betook myself to the railroad station to wait from 9:30 P. M. to 3:30 A. M. for the only train that would get me to Columbus in time for the *Journal* dinner. The train was good enough not to be late, and, by dressing for dinner before reaching Columbus, I, too, was on time to dine with a group of friends in whose company I seemed very small and unworthy: twenty of the contributors to the Commemorative Volume (a title that made me feel like a blessed shade). Most of those who contributed were asked to do so because I had been associated with them on one or another of the psychological journals. Dr. Bent-[p. 357]ley presided, Dr. Pillsbury spoke for the *American Journal*, Dr. Yerkes for the *Journal of Comparative Psychology*, Dr. Langfeld for the *Psychological Review*, and my former colleague, Dr. Helen Mull, for Vassar, while Dr. Warren presented the volume in the wittiest speech of the evening. Dr. Dallenbach was the moving spirit of all.

A conference of experimental psychologists was called by Dr. Dunlap at Carlisle, Pennsylvania, in March, 1928, to consider ways of advancing experimental research: I did some preparatory work for this in analyzing the results of a questionnaire on the equipment of the various psychological laboratories which Dr. Dunlap had sent out in order to get a basis for choice of the institutions to be represented at the conference; and also the latest reports of the various foundations to find what percentages of their gifts had gone to pure science. Needless to say, the percentages were very small indeed. Of the several suggestions adopted at this conference, that of the formation of a National Institute of Psychology, with headquarters at Washington, is now being carried out. It reminds some of us of the lapse of years; active members of the Institute will automatically become associate members of the age of sixty. Another recent conference called under Dunlap's direction was of editors and publishers of psychological journals. Various matters were profitably considered; for example, an excellent set of rules for the preparation of manuscripts by authors was formulated. Unfortunately, it takes more time to make an author follow rules than to correct his manuscript oneself.

In the spring of 1928, having developed some fatigue symptoms, I took the only leave of absence I have ever had and went on a Western Mediterranean cruise, my first trip abroad since my fourteenth summer. Appetite thus being whetted, in the summer of 1929 I spent a

fortnight in England.

The Ninth International Congress of Psychology, September 1-8, 1929, is still fresh in our memories; in mine it lingers as a recollection of talks with old and new friends, whether sitting on benches in the beautiful Harkness Quadrangle or at tables where we enjoyed the super-excellent food of the Yale cafeteria. I am sure our foreign friends will never forget the revelation of democracy in action which they obtained from standing in line and collecting their own sustenance at that cafeteria. I was elected to the International Committee at this meeting, an honor I appreciated the more because of the other Americans chosen at the same time.[p. 358]

One of the difficulties in writing these recollections has been that the present is so much more interesting than the past. It is hard to keep one's attention on reminiscence. Scientific psychology in America -- though not, alas! in Germany, its birthplace -- seems fuller of promise than ever before. The behaviorists have stimulated the development of objective methods, while configurationism is reasserting the importance of introspection; and, best of all, pure psychology is enlisting young men of excellent ability and a far sounder general scientific training than that possessed by any but a few of their predecessors.

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